

speech to which they belong. To learn to speak a foreign language by reading a grammar and writing exercises is an impossibility. We must imitate the procedure of the child, and be content to follow the same method in learning a new language that we followed when learning our own. The essence of a language is its idioms; no amount of grammatical study will teach us these. The study of grammar should come after our acquisition of a language, not before it.

M. Gerard defines his method as follows:—"We must accustom ourselves to the expression of ideas in the language we wish to learn by comparing it with their expression in our own, until we are able, through imitation and analogy, to express them in our own. In other words, we must understand the language and think in it before we use it." Understanding a language means reading and hearing it; using a language means speaking and writing it. Hence the course of study recommended by M. Gerard comprises the four distinct processes of reading, hearing, speaking, and writing, reading coming first and writing last. If reading is the primary object in learning a new language, M. Gerard's course is undoubtedly the right one, but if speaking is rather aimed at, we think it a mistake to make reading precede. What is heard will then have to be translated into the language of the eye before it is understood, and this will be a serious impediment to the learner. Moreover, a language consists in the phonetic sounds by which it is conveyed, not in the symbols whereby these sounds are expressed on paper. Learning to read should follow learning to speak, as it does in the case of children. With this single exception, we can heartily endorse all M. Gerard's recommendations; they are founded upon nature and reason, and their practical efficiency has already been proved. Especially noticeable are his remarks on the use of translations; a dictionary is desirable only when we have acquired a fair elementary knowledge of a language and its forms of expression. Language starts with the sentence, not with the isolated word.

A. H. SAYCE

OUR BOOK SHELF

The School Manual of Geology. By J. Beete Jukes, M.A., F.R.S., late Director of the Geological Survey of Ireland. Third Edition, revised and enlarged, edited by A. J. Jukes-Browne, B.A., F.G.S. (Edinburgh: A. and C. Black, 1876.)

THE late Prof. Jukes's admirable "School Manual of Geology" is already so favourably known to teachers of the science, for the clearness of its style, the accuracy of its information, and the abundance and excellence of its illustrations, that, in welcoming the appearance of a third edition of the work, we shall confine ourselves to a few remarks upon the changes which the editor has found necessary to make in it. In doing so, we have again to commend Mr. Jukes-Browne's skill in so well maintaining the distinctive characters of his uncle's work, while not hesitating to introduce such new matter as is demanded by the progress of the science.

In revising the chapter on igneous rocks, the editor acknowledges the assistance he has received from the Rev. T. G. Bonney. The principle of classification which he adopts—that, namely, of grouping the rocks, not according to one set of characters only, but on the basis both of their mineralogical constitution and their minute structure—we consider unexceptionable. To some of the definitions adopted in this chapter we must however de-

mur, as for example to those of andesite, porphyrite, and diorite, in all of which the essential felspar is stated to be *oligoclase*. As petrographers are not in possession of any ready means for determining the exact variety of felspar in a rock, in the absence of a complete chemical analysis of it, such a distinction becomes almost entirely useless in practice. Most continental writers avoid this difficulty by applying the same general terms to all such rocks as are shown, by microscopic examination or otherwise, to have any variety of the plagioclase felspars as their predominant constituent. We must also confess to grave doubts as to whether the revival of the obsolete term *leucilite* is warranted either by necessity or convenience.

In respect to that long-vexed question of geology, the limit between the Silurian and Cambrian systems, we think that Mr. Jukes-Browne has exercised a very wise discretion. He has in the present edition adopted the judicious compromise between the claims of Murchison and Sedgwick, which was long ago suggested by Lyell and Phillips, and has received such able support from the researches of Salter and Hicks. If convenience and scientific truth are not to be wholly sacrificed to the desire to do homage to the memory of an individual, it is quite time that the aggrandised empire of Siluria should be resolved into its proper elements, and that these should resume their due place in the brotherhood of formations.

In introducing some necessary changes into the chapter on the Glacial period, the editor has wisely avoided too hastily adopting any of the crude speculations which have recently been advanced on the subject. The statement, however, that the till of Scotland is of *older* date than the boulder clay of the English Midland Counties surely stands in need of some modification.

We heartily congratulate the editor and publishers of this very useful little manual on the well-merited success which it has attained.

Geology: its Influence on Modern Beliefs. Being a Popular Sketch of its Scientific Teachings and Economic Bearings. By David Page, LL.D., F.G.S. (Edinburgh and London: William Blackwood and Sons, 1876.)

UNDER the above title Dr. Page has published two essays which are devoted to an exposition of the chief scientific results, and a vindication of the economic value and importance, of geological research. The somewhat rhetorical style of these essays is sufficiently accounted for by the fact that they were originally prepared by their author as popular lectures for an Edinburgh audience—a disposition of them which was frustrated by his ill-health. Dr. Page has very effectively grouped, and eloquently sustained his several theses, while many of the chief points of his discourses are rendered more telling by admirably chosen illustrations from the immediate neighbourhood of the city in which the lectures were to have been delivered. In one or two instances, however, we notice that the author has not succeeded in avoiding the danger of making his generalisations of too sweeping a character—as for example when he informs us, without any qualification, that "men need not search for the veined marbles of the metamorphic rocks in tertiary beds, for metalliferous veins in secondary strata, nor for workable coal-seams in the Old Red Sandstone and Silurian systems."

The Law of Storms Considered Practically. By W. H. Rosser. (London: Chas. Wilson, 1876.)

WE have read this little book with very great pleasure, and can strongly recommend it to the navigator as giving briefly, but pleasantly and intelligently, an account of the history of the law of storms, down to the present time, inclusive of the various theories which have been propounded. The book is also to be commended as evincing throughout a remarkable justness of criticism, of which

the criticism on Prof. Blasius' recent book on storms may be cited as an illustration, and a close adherence to its text, viz., storms practically considered.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Force

IN his valuable lecture on force at Glasgow, reported in NATURE, vol. xiv., p. 459, Prof. Tait did great service by insisting on the duty of precision and consistency in the use of this as of other scientific terms, and showed clearly how the word "force" may be used precisely and consistently. My reason for troubling you with this communication is that I am unable to identify this use of the word with Newton's, on the assumption that the English equivalent for Newton's *vis* is "force."

As the same difficulty has probably occurred to other readers of NATURE, I should be glad if Prof. Tait would kindly tell us through your columns what are the equivalents in English for the phrases (1) *vis*, (2) *vis insita*, (3) *vis impressa*, each of which is used in Newton's "Principia."

In the phrase *vis insita*—if *force* is the English for *vis*—is not a meaning of the word "force" implied which is wider than and inclusive of the meaning of *vis impressa*? P. T. MAIN

An Intra-Mercurial Planet

THE discussion as to the existence of a planet within the orbit of Mercury leads me to communicate an observation made many years ago, which I believe nothing but the existence of an unknown planet between us and the sun can explain. On Sunday, January 29, 1860, the sun rose in a fog in London, so that he could be steadily looked at as if through a dark glass. Soon after eight o'clock a perfectly round black object was seen by four persons, including myself, clearly defined upon the lower half, according to my recollection, of the sun's disc. It passed slowly across his face and made its egress at about half-past nine A.M. In apparent size it was equal to the representations I have seen of Mercury in transit. F. A. R. RUSSELL

Pembroke Lodge, Richmond Park, September 30

Brilliant Meteor

THE brilliant meteor of September 24 was well seen in the neighbourhood of Ipswich, and as the observation of it was difficult in the absence of stars, the following notes may be useful. It was first seen at 6h. 31m. 15s. L.M.T., and the train was visible as a luminous cloud until 6h. 47m. 3s. L.M.T. The course had a length of about 25°, which was described in three seconds, and made an angle of 80° with the horizon. By means of the train which it left behind, it was possible to fix the point of disappearance with considerable accuracy, namely: altitude, 14° 6'; azimuth reckoned from south towards east, 54° 16'. At this time Saturn was visible, having an altitude of 10° 56', azimuth 53° 15'.

For purposes of description the course may be divided into three portions, roughly equal. In the first portion the meteor had a uniform brightness somewhat greater than a first magnitude star, but during the second portion it rapidly increased to many times the brightness of Venus, and almost suddenly diminished to its former magnitude. In the third portion it again increased in brilliancy, considerably exceeding its former maximum, and was suddenly extinguished without bursting. This third portion only was marked by the train estimated about 6° long, with a scarcely perceptible breadth. During the sixteen minutes that the train was visible it drifted about 12° northwards, losing gradually its definite outline. Direction of wind, south-south-west.

The diameter of the disc was certainly not greater than 2', and the form was pear-shaped, though not very prolonged, leaving the observer with the idea that the peculiarity of form was merely due to the persistence of the impression on the retina. It is very difficult to estimate its maximum brightness accurately, as the heavens afford us no object with which to compare it. I have recently shown that Venus has only $\frac{1}{80}$ th part of the light of the full moon, and there is no other standard

of light with which to bridge over this gap. If the moon had only a diameter of 2', its intrinsic lustre would be 240 times greater than it is, and the intensity would probably be such as would cause the observer involuntarily to avert his eyes when seen suddenly, even in full twilight; still, I do not think the meteor had much less light than such an object would have. The glare was of the colour, and closely resembled, a very vivid flash of lightning, for which it was mistaken by many persons.

JOHN I. PLUMMER

Orwell Park Observatory, September 27

The Age of Palæolithic Man

IN the extremely interesting communication on this subject which Mr. Skertchley has made to NATURE, vol. xiv. p. 448, there are one or two points on which I should like to say a few words.

First, in approaching this subject and endeavouring to find out the whole truth let us in starting have nothing but the truth. A human bone, a fibula, was certainly found beneath glacial clay in the Victoria Cave at Settle, but so far *no implements* have turned up from that ancient horizon. This is a simple inadvertence which does not in any way affect the strength of Mr. Skertchley's position, but I am anxious to correct it and as it were strangle it at the birth lest cuckoo-like it should shoulder kindred but legitimate statements out into the cold.

Mr. Skertchley's remarkable discovery consists in the finding of palæolithic implements beneath the great chalky boulder clay of Mr. Searles V. Wood, jun., which is the so-called East Anglian upper boulder clay, and this, as Mr. Skertchley says, and as I believe Mr. Searles Wood holds, and with which I certainly agree, is probably as old as the Lancashire lower boulder clay or till. And this Lancashire till is undoubtedly of the same age as the till of Scotland, as all authorities admit. Moreover this till is generally admitted to be the product of the great ice-sheet of Scotland and the North of England. We are therefore landed at the conclusion that implements have been found in beds which are probably of earlier age than the Scottish ice-sheet, a conclusion in which I cannot but heartily concur. Mr. Skertchley does not state this directly, but I presume this is the legitimate inference to be drawn from his statements, and one which he would himself admit.

There can be no doubt that this is very strong and corroborative evidence of the general views so ably urged by my friend, Mr. James Geikie, that all palæolithic implements and the fauna associated with them are of inter-glacial age. It may seem captious after having been led to the battle by so able a general, and having driven the enemy so far [already, to grumble at his stopping short in the pursuit, yet such is the object of my present remarks. And I would wish to point out that there are heights, or rather depths, which may yet be advantageously scaled to the further discomfiture of the foe.

Mr. J. Geikie has not ventured to carry the age of the bulk of the palæolithic beds further back than the time immediately *succeeding* the great Scottish ice-sheet. He appears to regard the "great submergence" which followed this as the chief cause for the removal from certain areas of the remains of men and animals which peopled them in inter-glacial times. "The palæolithic gravels of the south-east of England . . . are contemporaneous with those ancient valley-gravels of Scotland which overlie the till and boulder-clay, and which are themselves partially rearranged and covered with marine deposits belonging to the time of the great submergence."¹ He certainly once "puts his hand to the (ice-) plough." "No doubt, however, portions . . . especially in the districts south of the Thames, may date back to the earlier warm periods of the glacial epoch, and thus be contemporaneous with the fresh-water beds in the Scottish till; while some may go back even to pre-glacial ages;" but he immediately "looks back" to the sea of the great submergence as the great destroyer of palæolithic records. "After the great ice-sheet shrank back and the till and boulder clay had been deposited, a land-surface existed, rivers flowed down the valleys, and plants and animals clothed and peopled the country. In Scotland the fluviatile deposits belonging to that period have been subjected to great denudation, but in one place at least they have yielded animal remains, frogs and water-rats. But if the country had never been submerged after the withdrawal of the ice from the low grounds, there is good reason to believe that the presence of the relics of palæolithic man and remains of the animals with

¹ "The Great Ice Age," pp. 482-g.